

## THE FOUR-FOLD WAY OF KNOWING

### The Varieties of Social Science Experience

IAN I. MITROFF and RALPH H. KILMANN

*The hard and fast impassible line which is supposed by some to exist between "emotive" and "scientific" language is a reflex of the gap which now exists between the intellectual and the emotional in human relations and activities. The split which exists in present social life between ideas that have scientific warrant and uncontrolled emotions that dominate practice, the split between the affectional and the cognitive, is probably one of the chief sources of the maladjustments and unendurable strains from which the world is suffering.*

John Dewey, "Theory of Valuation"

There can be little doubt that what we have come to call scientific method has undergone significant transformation and development in this century and the last. Nowhere has this been more apparent than in the social sciences. If the essence of method can be identified with control, quantification, and measurement, then the ability of the social sciences to achieve these aims and thereby presumably to obtain method has increased considerably and continues to grow significantly. One of the basic theses of this paper is that the social sciences are on the verge of a wholly new and different type of revolution and development in method.

Unlike previous efforts which were focused almost exclusively on achieving quantitative growth and control, these newer developments depend upon the fashioning, juxtaposition, understanding, and ideally, the eventual integration of qualitative with quantitative methods of inquiring. This paper attempts to turn the social psychology of science on its head. We believe that our understanding, both empirical and theoretical, of the cognitive styles of inquiry that different scientists manifest in their day-to-day practice of science has

reached the point where we can now describe different methodologies of social science in terms of the differing orientations of social scientists. This is not thereby to reduce all matters of scientific method to "mere matters of psychology and sociology" exclusively, thus warranting the labels of "psychologism" and "sociologism". Rather, it is an attempt to explicate and to understand those features of scientific method which are an exemplification of the personalities (styles of inquiry) and social environments of social scientists.

Our motivation for such an effort at this time is the fact, we believe, that the tension, divergence, and, in some cases, open hostility and conflict between different styles and traditions of thinking about and practicing social science have reached the point in Western social science (and society at large) where these differences can no longer be ignored with impunity.<sup>1</sup> Indeed, whenever fundamental differences in style and orientation have persisted for so long, it not only behooves us to take them seriously but to attempt to under-

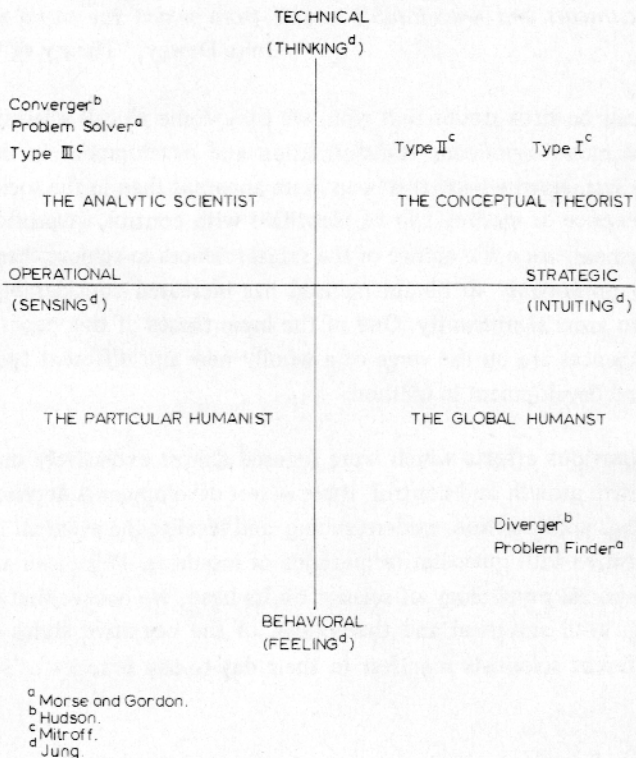


Figure 1. A typology of typologies of scientific inquiry styles.

stand the reasons underlying those differences. A central thesis of this paper is that a major *part* (but only a part) of the differences can be understood in terms of psychological and sociological factors. In short, where in the past we have used a method to study psychological and sociological factors, we now feel we can use knowledge of psychological and sociological factors to study method. The purpose is not only to gain a unique perspective on some old (traditional) methods of social science but to gain a glimmer (however tentative) of some newly emerging methods. Before we can do this, however, we need to develop a framework whereby we can discuss systematically the set of social psychological factors upon which our discussion of method rests.

### A Typology of Typologies of Scientists

Through examination of some of the few typologies that currently exist with regard to different cognitive styles of inquiring we have extracted a common set of dimensions as shown in Figure 1, a "typology of typologies". It can be demonstrated that the dimensions are sufficient to plot or compare the individual typologies within a common framework.<sup>2</sup> Since the typologies were developed independently of one another, the fact that they are all tapping in to the same common set of dimensions lends credibility to the overall framework. We must emphasize that the placement of the various types or styles of inquiry is meant only to be relative. The figure is not meant to be read that Convergents, for example, are exactly five units beyond Divergers. The figure is intended to convey that Convergents and Divergers represent diametrically opposite ways of inquiring and for this reason occupy diametrically opposite cells or relative locations of the figure. No less important is the fact that Hudson's Converger,<sup>3</sup> Morse and Gordon's Problem Solver,<sup>4</sup> and Mitroff's Type III scientist<sup>5</sup> all essentially represent the same cognitive style of inquiring. The Diverger and the Problem Finder also represent a similar orientation but one which is very different from that of the Converger, Problem Solver, and Type III scientist.

Because there is correspondence between the types of the different typologies, we have found it convenient for discussion purposes to refer to four basic "types" or kinds of scientists: (1) the Analytic Scientist (AS); (2) the Conceptual Theorist (CT); (3) the Global Humanist (GH); and (4) the Particular Humanist (PH). Our choice of labels for these different types will, we hope, become clear as we proceed. In discussing the psychological profiles or meaning of each of these four types, we refer to the two orthogonal dimensions (1) Operational versus Strategic, and (2) Technical versus Behavioral. Each of the typologies we have examined makes a fundamental

distinction between operational versus strategic and technical versus behavioral styles of thinking. While there are scientists who can do both (for example, scientists who can perform equally well on both ends of the operational-strategic dimension), that is more the exception than the rule.<sup>6</sup> Generally, most scientists tend towards one end of the spectrum or the other. Further, there are both strong theoretical<sup>7</sup> and empirical reasons for asserting that if one is engaged in operational thinking or concerns then *at the same time* one cannot be engaged in strategic thinking or concerns. Or, differently stated, the farther one is located out on the extremities of either end of the spectrum, the less one is able to perform or to appreciate the opposite type of skill or way of thinking. In short, the ends of the dimensions are describing very different styles of thinking in social science.

Finally, before describing each of the dimensions underlying the four types, one last caveat: by the notion of a "type" we do not mean to imply that every scientist fits neatly and literally into one and only one of our four styles. While there is a tendency for most scientists to develop a marked preference for one end of each of the two dimensions, most scientists are nevertheless complex mixtures of both ends. Further, most oscillate back and forth over the course of their careers as the situation and the problem dictates. The present typology pertains more to the statics of science than its dynamics. The dynamics of the system is a fitting topic of its own. We do not also mean to imply that four and only four types are sufficient to exhaust the full range of the richness, variety, and complexity of the scientific species. As one of our mentors, Thomas Cowan, was fond of putting it, "There are two kinds of people in this world: those who think there are two kinds of people and those who do not." If we ourselves are a type, we hope it is of the latter variety and not of the former. Our purpose in putting forth a four-fold typology is certainly not to reify the scientific spirit, but rather to make possible a critical discussion of some important social psychological dimensions which can be gleaned in the day-to-day workings of science and which in our opinion cannot be overlooked. If we have reasons for being suspicious of those who would uncritically embrace a typology, we would be equally and perhaps even far more suspicious of those who reject out of hand the concept of a typology and its usefulness *per se*. The main purpose of our effort is to make possible a critical discussion of a most elusive and difficult-to-study phenomenon — the scientific personality.

A major distinguishing characteristic of operationally-oriented scientists is that they are engaged in what Dewey called a pursuit or "quest for certainty".<sup>8</sup> Once this basic characteristic is recognized and appreciated, a number of seemingly diverse attributes fall into a recognizable and coherent pattern. Thus, it is not enough to acknowledge that operational scientists are often

fine and detailed experimentalists, but that they exhibit a marked and strong belief in experimentation as a basic "philosophy of research or science". To the operationally-minded scientist, unless a problem, question, or issue can be defined precisely, broken down into its constituent parts or elements, and "hard," objective (reliable, valid) data gathered on the parts, he is likely to consider the question as meaningless, not a fitting topic for scientific inquiry. This type of scientist is adverse to anything which cannot be posed or settled precisely and unambiguously. Accompaniments of this attitude are the operational scientist's belief in reductionism, precision, the need for certainty and closure, in sticking close to the available data and only to what can be observed. Correspondingly, he often exhibits a marked and even hostile aversion to open-ended, speculative questions and inquiries. In short, the operational scientist is governed by his strong belief in the testimony of the senses which, when translated into modern science, means a marked preference for gathering hard, objective data. Other characteristics include his "real-time orientation"; the operational scientist tends to operate in the "here-and-now". In part this follows from his need for and belief in breaking problems into manageable parts, each of which can either be researched or solved independently of the others. We call this type of scientist "operational" because of his need and desire to apply relatively well-understood and well-accepted procedures and techniques to problems which can themselves be viewed as or are well-structured. The operational scientist is more interested in the testing, verification or falsification of well-structured (already known or given) hypotheses than he is in either the discovery or the formulation of new hypotheses.

The strategically-minded scientist represents the complete or nearly complete opposite frame of mind. Where the operational scientist is governed by the need to break problems and issues down into their elemental parts, the strategic scientist is governed by his need to perceive problems as part of a larger global "whole" or framework. Where the operational scientist is a realist who operates in the "here-and-now," the strategic is an idealist who operates in or lives for the future. Where the one eschews speculation, the other embraces it as a characteristic method. Where the one is adverse to ambiguity, the other is not only not disturbed by it but perceives it in a positive light; for him, it offers an opportunity to be creative and inventive, to open up new possibilities. Where the one believes in sticking close to certainty (hard, objective data), the other believes in extrapolating as far as he can beyond the available data in order to set the stage for new inquiries. Where the one believes in the search for a single best (optimal) answer to a question (and correspondingly that there is a single best way of posing a question), the other believes that there are multiple possible ways of posing and responding to any question.

The differences between these two types can be summarized as follows. When the operational scientist entertains multiple possibilities it is only for the purpose of eventually reducing them down to a single best explanation. For the strategic scientist, the construction of multiple possibilities is not merely something to be tolerated; it is the very essence of scientific inquiry. It is vitally important to appreciate that neither of these types is necessarily better or best for all situations. Indeed, each occupies a valuable place in the house of science. Since each picks up a needed aspect of science which the other ignores or is insensitive to, they each need and depend upon the other far more than they often realize.

As much as any one of whom we are aware, Abraham Maslow stressed the need for each of these two types (extremes) to learn how to get on with one another.<sup>9</sup> Maslow defined the "healthy" scientist as the rare individual who was able to combine both ends of our various spectrums:

It is possible for healthy scientists to enjoy not only the beauties of precision but also the pleasures of sloppiness, casualness, and ambiguity. They are able to enjoy rationality and logic but are also able to be pleasantly crazy, wild, or emotional. They are not afraid of hunches, intuitions, or improbable ideas.

Ultimately, I am convinced, we shall have to include in the education of the young scientist both the techniques of caution and of boldness. Mere caution and soberness, mere compulsiveness can produce only good technicians who are much less likely to discover or to invent new truths or new theories. The caution, patience, and conservatism which are *sine qua non* for the scientist had better be supplemented by boldness and daring if creativeness is also the hope. Both are necessary. They need not be mutually exclusive. They can be integrated with each other. Taken together they constitute flexibility, adaptability, versatility.<sup>10</sup>

As we read Maslow, the challenge is to develop a concept of science and scientists which understands that the two ends need not be mutually exclusive and who know psychologically how to integrate both ends. The needed integration Maslow is talking about pertains as well to the other dimension of our framework, the technical-behavioral dimension. Before discussing the ends of this dimension, it is important to appreciate the differences between the two dimensions. In a number of senses, the operational-strategic dimension corresponds to a data-input dimension; the two ends of this spectrum refer to different ways of assimilating data, if not fundamentally differing conceptions of data. The preferred data of the operational scientist are facts, sensations, pointer readings, numbers, and like. While it is hard for many scientists to appreciate it, the preferred data of the strategic scientist are speculative, hypothetical *possibilities*. For those who have been weaned on

the notion of hard data, it is difficult to appreciate that possibilities *are* data of a certain kind. For the strategic scientist, possibilities are not merely as real as "objective data," they are the stuff of which reality is made. In discussing these two types, we are talking about two different psychological realities or world-views.

If the operational-strategic dimension corresponds to a data-input dimension, then the technical-behavioral dimension corresponds to a decision-making dimension — to two different ways of responding to the input-data, two different ways of ordering reality. The ordering principles of the technically-minded scientist are based upon impersonal systems of logic and analysis. In this world-view, things are ordered and evaluated according to their logical character or truth-content; personal considerations play no role in the ordering of objects, persons, and events or with regard to their evaluation. Ordering and evaluation is or should be done on a purely impersonal basis. The ordering principles of behaviorally-oriented scientists are based upon personal value systems of morality and ethics. Where the technical is concerned with the world of impersonal ideas, concepts, and data, the behavioral is concerned with people and feelings.

It would take us too far afield to explore the many subtleties and nuances of these two ways of ordering reality. Two aspects in particular are especially worth mentioning since they will concern us shortly when we explore the methodological implications of the typology. The dimension technical-behavioral corresponds most closely to the Jungian personality dimension of Thinking versus Feeling.<sup>11</sup> In Jungian terms, Thinking is the psychological function that generalizes across particulars impersonally. Thinking is the psychological function that dispassionately places objects, persons, and events into impersonal frameworks and then analyzes them in terms of their place within that framework. Indeed, according to this view, a person, object, or entity only takes on meaning by virtue of its being assigned some place within some framework; i.e., by being placed into an abstract, impersonal, theoretical class. Feeling, on the other hand, is the psychological function that individuates. It seeks to find what is uniquely characteristic, different, and special about *this particular* person, object, or event. It seeks to treat every person as a unique end and being unto himself, not as an impersonal means. Thus, whereas Thinking asks how something serves the overriding aim of approaching closer and closer to the Truth, Feeling asks how something serves the overriding aim of increasing individual human welfare and happiness. Whereas the first and the last concern of Feeling is people, the first and last of Thinking is impersonal logic.

Combining the attributes of each end of both dimensions in all possible ways results in the four-fold typology in Figure 1. Thus, for example, the Analytic Scientist (AS) combines the attributes of the operational and technical scientist: a preference for collecting detailed, specific data on an issue and analyzing it impersonally and logically. The Global Humanist (GH), on the other hand, exhibits a marked preference for inventing global and speculative possibilities whose purpose is to further broad, far-reaching human goals. The framework thus helps to explain the tension and divergence between two different pairs of contrasting types of scientists, the AS and the GH, and the CT and PH. On each of the two dimensions, these two sets of scientists are maximally opposed. They share no common psychological ground; for instance, the CT and GH share at least one psychological function in common (strategic thinking), while the CT and PH share nothing in common. We turn now to a discussion of the methodological implications of this framework.

#### Four Methods of Social Science

In this section we wish to show the social psychological qualities of mind (personality attributes) upon which the characteristic methods of each type rests. For example, to the extent that the AS glorifies *disinterested knowing* as a basic prerequisite for scientific inquiry, we should expect to find this quality strongly mirrored in his methods. To the extent that the CH, on the other hand, glorifies *interested involvement and interpersonal caring*, we should expect to find this quality reflected in his methods.

##### *The Analytic Scientist*

While the preferred method of the AS assumes a wide variety of different shapes and forms, the best descriptive class label is *controlled inquiry*. Since the range of methods that can be subsumed under this class is so broad, it proves most convenient to illustrate the nature of the class with a single well-chosen example. In many senses, there is no more representative example of the controlled inquiry than the controlled experiment. The concept of the controlled experiment is not only representative of the general class of methods subsumed by the AS, but it often seems as if the controlled experiment is important as the archetypal representative of this class of inquiry for two reasons: (1) its modern roots are relatively recent; thus its historical and philosophical underpinnings can be traced back with "relative ease"; and (2) in spite of the tremendous development in the technical details of the controlled experiment, the basic underlying notion remains essentially the same as when it was first formally codified.



A landmark in the history of the controlled experiment is represented by Mills's Canon of Induction.<sup>12</sup> In particular, Mills's Canons pretended to offer the empirical scientist a set of formal procedures whereby he could discover if two events X and Y were causally connected. That Mills's procedures failed because they contain serious defects<sup>13</sup> which prevent them from being as conclusive as Mills thought need not detain us here. Mills, however, contributed significantly to the notion of experimental thinking; indeed, he put it on a course that directs it to this very day. If anything, the defects of Mills's Canons are instructive and important in themselves for they reveal the generic difficulties which plague all experimentation.

Because the primary purpose of this section is to describe the characteristic methods of the AS and the other types, it is beyond our scope to engage in a systematic and detailed discussion of Mills's Cannons. For our purposes, it suffices to discuss Mills's second Canon, the Method of Differences, in order to see how it embodies the AS's preferred method and how it has undergone significant transformation since it was first proposed. Suppose an experimenter (E) suspects that two events X and Y are causally connected. Mills's second Canon directs an E to observe that whenever (if) Y (the presumed cause) is present along with the intervening events or secondary causes (A, B), the presumed effect X follows. In somewhat more formal terms:  $Y, A, B \xrightarrow{?} X$ . The second Canon also directs an E to observe that whenever (if) Y is absent ( $\bar{Y}$ ), X is also absent ( $\bar{X}$ ). A more formal expression for this is:  $\bar{Y}, A, B, \xrightarrow{?} \bar{X}$ . If these conditions are met (i.e., if the question marks are removed), then presumably the *necessity* of Y for X is established.

Whether the method really works or not in the way Mills thought it could is not the important issue at hand here. What is important is that it clearly reveals the AS's perpetual concern and preoccupation with such values as precision, control, specificity, exactness, and most of all, the distrust and avoidance of ambiguity. If the Canon is to work at all, then one must be able to specify and enumerate exactly and completely all those factors (potential causes, Y, A, B. . . .) possibly affecting X. "To know," in other words, is for the AS synonymous with controlled and systematic inquiry — the precise, systematic enumeration of the potential causes or factors affecting an outcome, property, or effect attached to an object of knowledge. While more modern formulations have altered significantly the appearance of the Canon, the underlying logic remains essentially the same. For example, in the framework of perhaps *the* single most influential contemporary treatise on the subjects, *Experimental and Quasi-Experimental Design*,<sup>14</sup> the second Canon can be expressed as follows:

$$\begin{array}{l} Y, A, B \longrightarrow \left| O_2 - O_1 \right| = X > 0. \\ \bar{Y}, A, B \longrightarrow \left| O_2 - O_1 \right| = X = 0. \end{array}$$

That is, the effect  $X$  is the difference  $O_2 - O_1$  between an initial observation  $O_1$  made *prior to* the administration of a treatment ("cause")  $Y$  and an observation  $O_2$  made subsequent *a posteriori* to  $Y$ . In the language of modern experimentation, the first equation describes the treatment group; the second the control group, the group that is not administered  $Y$ . Given these two equations it becomes relatively easy to appreciate the AS's concerns with precision and removal of ambiguity. Unless one can define and measure  $Y$ ,  $A$ ,  $B$ , and  $X$  as precisely and as unambiguously as possible, it becomes difficult, if not impossible, to distinguish between the treatment and control groups. In more technical terms, it proves difficult to eliminate potential confounding effects. Unless, for example, one can clearly differentiate  $Y$  from  $A$  and  $B$ , one cannot determine whether  $Y$  is necessary or not to the presence of  $X$ . In the language of Campbell and Stanley, one has not controlled for potential threats to internal validity, factors other than  $Y$  which if not controlled for can be mistaken for causing  $X$ .

These two equations are important for illustrating other aspects of the method of the AS. In particular, they suggest an important measure of performance associated with the AS's method. In statistical decision theory it is customary to define two errors associated with hypothesis-testing; the errors of the first and second kinds or  $E_I$  and  $E_{II}$  respectively. These errors are defined with reference to the two hypotheses  $H_0$  and  $H_1$  where  $H_0$  is defined as  $O_2 = O_1$  and  $H_1$  is defined as  $O_2 \neq O_1$ .  $H_0$  is commonly referred to as the null hypothesis.  $H_0$  thus makes reference to the first equation;  $H_1$  to the second. Formally,  $E_I$  and  $E_{II}$  are defined as follows:  $E_I$   $\stackrel{\text{df}}{=}$  Probability (Rejecting  $H_0/H_0$  presumed true)  $E_{II}$   $\stackrel{\text{df}}{=}$  Probability (Rejecting  $H_1/H_1$  presumed true). So defined, the method of the AS is to define and conduct precisely and systematically at least two experiments as given by our two equations so that, as much as is possible,  $E_I$  and  $E_{II}$  can be minimized or, conversely, so that  $1-E_I$  and  $1-E_{II}$  can be maximized.

It is beyond our scope here to treat in detail another characteristic feature of the AS method. Of necessity, we can merely mention in passing that the logic of the AS is Aristotelian. This can be illustrated by noting that for the AS it cannot be the case that both  $H_0$  and  $H_1$  are true, or alternately, that both can be accepted or rejected at the same time. Acceptance of  $H_0$  implies rejection of  $H_1$  and rejection of  $H_0$  implies acceptance of  $H_1$ . Thus, it cannot be the case that Reject  $H_0$  and Reject  $H_1$  since this is equivalent to Reject  $H_0$  and Accept  $H_0$  or Accept  $H_1$  and Reject  $H_1$ . In more formal terms this can be expressed by saying that the AS not only accepts but insists upon the law of contradiction [not ( $p$  and not  $p$ )] as a basic postulate (characteristic) of the laws of thought, logic, and even reality itself. That is to say, the structure of

reality is logical, and that it obeys the law that a proposition and its negation cannot both be true at the same time.

### *The Conceptual Theorist*

The CT shares with the AS his impersonal and theoretical orientation to social phenomena. Like the AS the CT is interested in the formulation of universally valid, impersonal, theoretical laws. The difference between them, however, lies — among many things — in the character, status, and functions they impute to the notion of a “scientific law”. For the AS something counting as a law must be capable of serving as a precise deductive and predictive instrument. Put somewhat differently, the AS believes the sole aim of science consists in the discovery and formulation of those laws which approximate more and more closely the “truth”. For the CT, on the other hand, there is no such thing as a single standard of truth, let alone a single, self-consistent, self-sufficient law which comes closer and closer to the truth. The purpose of theorizing for the CT is not to arrive at a single, all-encompassing, “correct” law, but rather to allow him to engage in his most pleasurable and exalted activity: conceptual model-building. Whereas the AS is interested in minimizing the type I and type II errors which are associated with hypothesis-testing, the CT is interested in minimizing a lesser known error which pertains to hypothesis-discovering — “the error of the third kind” or E<sub>III</sub>.

E<sub>III</sub> has been defined as “the probability of solving the ‘wrong’ problem when one should have solved the ‘correct’ one.”<sup>15</sup> While the technical details of the computation of E<sub>III</sub> are beyond the present paper, it is important to emphasize that E<sub>III</sub> involves attaching a probability measure to the various ways in which a problem can be conceptualized. It is vitally important to emphasize that the concept of E<sub>III</sub> does *not* entail determination of the strict “wrongness” or “correctness” formulation of a problem in any absolute sense. (Were this to be the case we would be back in the hands of the AS, only at a different level of analysis.) Rather, E<sub>III</sub> involves the *relative* determination of “correctness” by asking what good does it do to attempt to solve a “wrong” formulation of a problem precisely. What good, after all, does it do to minimize E<sub>I</sub> and E<sub>II</sub> if the hypothesis to begin with is faulty (i.e., if E<sub>III</sub> is high)? To minimize E<sub>I</sub> and E<sub>II</sub> at the expense of E<sub>III</sub> is to commit the fallacy of misplaced precision, to achieve precision at the expense of the relevancy and validity of the question being asked.

As much as any single work of which we are aware, a relatively recent article by Murray Davis, “That’s Interesting!, Towards a Phenomenology of Sociology and a Sociology of Phenomenology,” represents a significant

step towards the codification of the CT's approach.<sup>16</sup> It has been harder to judge scientific ideas on CT grounds and to practice science in a CT mode because compared to the AS approach the CT approach is still relatively uncodified. The CT approach, with its primary emphasis on hypothesis-formulation, is much harder to codify than the AS approach, with its primary emphasis on the testing of already discovered (formulated) ideas. Unlike the AS approach, there is no hypothetical-deductive method, in the sense of a set of well-formed rules, for practicing CT. Davis's work is a promising step in this direction. Davis starts by advancing a number of bold theses. The first is that the great social scientists were not great because they produced "true" theories. Being simplifications, all theories necessarily become false at some point. While it may make sense to think of some theories as being more or less false than others, Davis stresses that *all* theories fall into the class of "false" entities in that the acceptance or utility of a theory (at least in the social sciences), is determined primarily on grounds other than truth *per se*. One of the main grounds for acceptance, Davis contends, is that of the "interestingness" of a theory.

It has long been thought that a theorist is considered great because his theories are true, but this is false. A theorist is considered great, not because his theories are true, but because they are *interesting*. Those who carefully and exhaustively verify [note that this is an AS trait] trivial theories are soon forgotten; whereas those who cursorily and expediently verify interesting theories are long remembered [note that if this is true then this helps to explain why the CT is often deemed by the AS as sloppy]. In fact, the truth of a theory has very little to do with its impact, for a theory can continue to be found interesting even though its truth is disputed – even refuted.<sup>17</sup>

The basic question then is: What is it that makes a theory or a theorist interesting? Davis's contention, which is interesting in itself (i.e., Davis himself has constructed an interesting theory with regard to "interesting theories" and thus satisfies his own methodology), is that an interesting theory is one which (1) identifies a previously taken-for-granted underlying assumption of a significant body of social theorists, (2) exposes, perhaps for the first time, the assumption as an assumption for critical and public scrutiny, and, most important of all, (3) argues forcefully why a counter-assumption is actually more plausible.

An *interesting* proposition [is] one which first [articulates] a phenomenological presumption about the way a particular part of the world [looks], and then [denies] this phenomenological presumption in the name of "truth," that is, in the name of a more profound, more real, more ontological criterion. Put more precisely, an *interesting* proposition [is] one which [attempts] first to expose the ontological claim of its accredited counterpart as merely phenomenological pretense, and then to deny this phenomenological pretense with its own claim to ontological priority. In brief, an

*interesting* proposition [is] always the negation of an accepted one. All of the propositions I [have] examined were easily translatable into the form: "What seems to be X is in reality accepted as X is actually non-X."<sup>18</sup>

As Davis notes, the process of assumption uncovering and denial is a tricky, complex social-psychological process. If a counter-assumption merely affirms instead of denies some aspect of an audience's set of background beliefs, then not only is the audience unlikely to find the counter-assumption interesting but they are likely to express this by saying, "That's obvious!" Alternately, a proposition or counter-assumption can be considered non-interesting if it does not speak to any aspect whatsoever of the audience's background beliefs. The response here is likely to be, "That's irrelevant!" Thirdly, if a counter-assumption denies the whole set of background beliefs, it is likely to be labeled as "That's absurd!" To repeat: assumption denial is tricky and complicated; deny too little and what one is doing is called trivial; deny too much and one is labeled a crackpot. The latter is often the fate of laymen who have claimed to have found or invented radical alternatives to accepted scientific theories. Such people tend to be dismissed outright by "serious" scientists. What Davis suggests is that the same fate can accrue to scientists who dare to challenge accepted ways. The history of science is replete with such cases.

Table I lists the categories whereby one can judge, according to Davis, the "interestingness" of a proposition. The categories are taken directly from Davis's paper. The reader is directed to that paper for liberal examples taken from the fields of psychology and sociology. The examples profusely illustrate that there are prominent cases for every category listed in Table 1; for example, if there is a prominent case which illustrates principle 1a in the table, then there is an equally prominent or dramatic case from the history of social science which illustrates principle 1b. Those who are familiar with Kant's categories for synthetic judgments will recognize some striking similarities with Davis's categories. Not only do Davis's categories include some of Kant's but they do so for a different purpose. Whereas Kant's categories were deemed necessary for the perception of physical reality — what was necessary for the mind to presuppose or contain *a priori* so the act of perception would thereby be "possible" — Davis's categories lay out the choices open to the active designer of a social inquiry that he must make if he chooses to engage in inquiry. Davis's categories are necessary because they embody these choices.

When Davis's principles are placed directly next to one another (e.g., 1a and 1b, 2a and 2b), we can see explicitly the *dialectical* nature of the task facing

TABLE I  
Categories for Judging the Interestingness of Propositions

<i>Single Phenomenon</i>	<i>Multiple Phenomena</i>
<p>1. Organization</p> <p>a. What seems to be a disorganized (unstructured) phenomenon is in reality an organized (structured) phenomenon.</p> <p>b. What seems to be an organized (structured) phenomenon is in reality a disorganized (unstructured) phenomenon.</p>	<p>8. Co-Relation</p> <p>a. What seem to be unrelated (independent) phenomena are in reality correlated (dependent phenomena).</p> <p>b. What seem to be related (interdependent) phenomena are in reality uncorrelated (interdependent) phenomena.</p>
<p>2. Composition</p> <p>a. What seem to be assorted heterogeneous phenomena are in reality composed of a single element.</p> <p>b. What seems to be a single phenomena is in reality composed of assorted heterogeneous elements.</p>	<p>9. Co-Existence</p> <p>a. What seem to be phenomena which can exist together are in reality phenomena which cannot exist together.</p> <p>b. What seem to be phenomena which cannot exist together are in reality phenomena which can exist together.</p>
<p>3. Abstraction</p> <p>a. What seems to be an individual phenomenon is in reality a holistic phenomenon.</p> <p>b. What seems to be a holistic phenomenon is in reality an individual phenomenon.</p>	<p>10. Co-Variation</p> <p>a. What seems to be positive co-variation between phenomena is in reality a negative co-variation between phenomena.</p> <p>b. What seems to be a negative co-variation between phenomena is in reality a positive co-variation between phenomena.</p>

4. Generalization
  - a. What seems to be a local phenomenon is in reality a general phenomenon.
  - b. What seems to be a general phenomenon is in reality a local phenomenon.
5. Stabilization
  - a. What seems to be a stable and unchanging phenomenon is in reality an unstable and changing phenomenon.
  - b. What seems to be an unstable and changing phenomenon is in reality a stable and unchanging phenomenon.
6. Function
  - a. What seems to be a phenomenon that functions ineffectively as a means for the attainment of an end is in reality a phenomenon that functions effectively.
  - b. What seems to be a phenomenon that functions effectively as a means for the attainment of an end is in reality a phenomenon that functions ineffectively.
7. Evaluation
  - a. What seems to be a bad phenomenon is in reality a good phenomenon.
  - b. What seems to be a good phenomenon is in reality a bad phenomenon.
11. a. What seem to be similar (nearly) identical phenomena are in reality opposite phenomena.
12. a. Causation
  - a. What seems to be the independent phenomenon (variable) in a causal relation is in reality the dependent phenomenon (variable).
  - b. What seems to be the dependent phenomenon (variable) in a causal relation is in reality the independent phenomenon (variable).

the social scientist or social experimenter. In structuring an inquiry, not only does the social scientist have to choose which principle governs his inquiry (e.g., principle 1 versus 7), but he also has to wage an internal dialectic as to which sub-principle applies (e.g., 1a, versus 1b). Consider, for example, principle number 12 and the difference between the AS and the CT approach to the choice between 12a and 12b. Let us say that 12a can be expressed in the spirit of the preceding section as  $X \longrightarrow Y$  and 12b as  $Y \longrightarrow X$ . The AS will reduce the problem to the "best" choice between 12a and 12b. He will be guided in his choice by the degree to which either 12a or 12b best fits in with the body of contemporary thought, ideas, and available data. He will also be guided in his choice by which schema can be most readily expressed in a form testable in accordance with the Campbell-Stanley framework outlined in the previous section. The AS will, in other words, reduce the problem to a single choice between 12a and 12b. In accordance with his system of logic, 12a and 12b cannot both be true nor false.

The CT, on the other hand, operates on a different intellectual wavelength. The CT will first identify which of the two, 12a or 12b, most accords with accepted thinking, theories, data, and facts. He will then try as hard as he can to see if a good (if not a better) case can be made for the opposite or least accepted schema in order to mount the strongest possible challenge to our most sacred, cherished, and commonly accepted ideas.<sup>19</sup> Only in this way does the CT feel he can shake us from our dogmatic slumbers and force us to confront what we have been taking for granted. And, in fact, the highest form of CT thinking will be constructing a dialectic between 12a and 12b. That is, what are all the good (best) reasons that can be given why 12a is a good representation of the two phenomena under discussion, and what are all the good (best) reasons why 12b is a good representation of the two phenomena under discussion. After the social scientist has done everything in his or her power to mount the best case for these two antithetical representations of the same phenomena we are attempting to explain, and if one way of representing them seems to keep coming up positive — only then (if ever) are we justified in accepting one schema over the other as a better representation of nature. In the extreme, we are merely enjoined to keep looking for other ways of expressing the opposition in order to keep the dialectic alive. The CT's outlook is not directed so much towards the resolution of conflict and the quest for certainty as it is for the toleration, proliferation, and enjoyment of ambiguity and of multiple ways of viewing the world.

Thus it is no random accident that we have chosen to label this outlook "conceptual exploration". Whereas the AS is oriented to finding which single schema best explains and represents the world in exact detail, the CT is



instead interested in exploring, creating, and inventing multiple possible and hypothetical representations of the world — even hypothetical worlds themselves. Further, the CT's emphasis is on the macro-differences between these different representations, not on the details of any single schema. In the extreme, the correctness and the details of any single picture are postponed indefinitely. If a potential danger of the AS is getting bogged down in details, a potential danger of the CT is ignoring them altogether for the sake of comprehensiveness. If the AS tends to suffer from "hardening of the categories" (his innate love for single, simple schemas), the CT tends to suffer from "loosening of the wholes".

### *The Global Humanist*

Global Humanism (GH) and Particular Humanism (PH) are very different in spirit, outlook, and temperament from the preceding two methodologies we have examined. As we have seen, while the AS and the CT differ markedly in the details and substance of their respective approaches, the emphasis in both cases is on the *impersonal and dispassionate* generation and evaluation of ideas, data, and theories. This is in sharp contrast to the emphasis of the GH and PH, which in both cases is on the *passionate and personal* generation, evaluation, and application of social science knowledge for the general betterment of man. For the AS and CT, the central, if not overriding, aim of science is furthering the increase in (1) our abstract, theoretical and (2) our concrete, factual or empirical knowledge. For the GH and PH the overriding aim is developing the kind of social science knowledge and methods which will further the development and attainment of individual human growth, self-awareness, and general welfare. In order to appreciate the unique methods of the GH and PH, it is necessary to understand that (if only in part) they are a reaction to the methods of the AS and CT. This fact alone helps to account for the relatively large space we have devoted to the AS and CT. The most general criticism leveled by the GH and PH against the AS and CT is that for all their technical sophistication and expertise in building theories, collecting data, and analyzing data, their methods are not only responsible for the collection of the wrong data regarding human behavior, but also for the production of the wrong data. In short, they contend that the methods of the AS and CT are themselves responsible for the distorted meaning, appearance, and representation of data pertaining to human behavior.

In a by now classic paper, "On the Unintended Consequences of Rigorous Research," Chris Argyris argued that the AS approach, with its heavy emphasis on the systematic enumeration and tight control of as many factors as possible affecting an experiment, fostered an artificial human environ-

ment.<sup>20</sup> Tight control of factors translated into tight restrictions on the varieties and types of behavior that were allowed to manifest themselves. Argyris argued that the behavior manifested under such restrictive circumstances and the knowledge derived therefrom ought to be applicable (generalizable) only to other such repressive and autocratic environments found in schools, prisons, mechanized assembly lines, and the armed services. Under such conditions, Argyris argued, it should not be surprising to find that research subjects exhibit such anti-social behavior as (1) withdrawal (alienation) from both the experiment and the experimenters, (2) deliberate wrecking of the experiment (withholding data, cooperation, etc.), and (3) forming adverse attitudes towards social science and social scientists (e.g., the fact that deception has often been used as an experimental technique has resulted in the expressed attitude among subjects that "psychologists always lie!"). The result is what Argyris and others have called the production of "behaviorally invalid data," a poor basis indeed upon which to erect generally valid theories of human behavior.

Specifically in order to correct such undesirable tendencies Argyris<sup>21</sup> and others<sup>22</sup> have proposed radically new methods of collecting behaviorally-valid data, and, even more to the point, have actively and genuinely involved subjects as total human beings in *all the phases* of a research project, not merely as passive stimulus-response mechanisms whose primary purpose is to behave or produce on demand. It is unfortunately way beyond the scope of this paper to describe any of these new techniques in the detail they deserve. One technique in particular, however, deserves some mention.

Recently Chris Argyris and Donald Schön have outlined a technique that captures the spirit of the GH approach as well as any procedure of which we are aware.<sup>23</sup> The technique is inherently dialectical but fundamentally unlike the dialectics of the CT's approach. Whereas the dialectic of the CT is based on the clash between abstract ideas, the dialectic of the GH is based on the clash between fundamentally differing all-too-human images an individual entertains about himself. The purpose of the GH's dialectic is not to further some abstract notion of truth but rather to help an individual achieve better self-awareness, growth, and personal control. Without delving further into the details of the procedure, suffice it to say that the dialectic arises through helping individuals confront the difference between what they *say* they do (their espoused theories of inter-personal behavior) and what they *actually do* (behave) (i.e., their actual theories-in-use). The purpose of such techniques is not only to help individuals achieve self-growth, but also to found *generally applicable theories* of human behavior built on the base of valid behavioral data. This concern with generality separates the GH from the PH.

### *The Particular Humanist*

One of the essential differences between the GH and the PH involves the unit of social reality each of them takes to be of primary concern. Both, as we have indicated, are genuinely and deeply concerned with people, values, morality, and ethics, and not with some depersonalized abstractions of them that fail to relate directly to people in a humanistic fashion. The ultimate aim of science, as we have repeatedly stressed, is, for the PH and GH, serving people, not some abstract concept of impersonal and timeless "truth". This much the GH and PH both share.

Where the GH is interested in discovering and formulating broad (general and holistic), humanistically grounded theories of social behavior applicable to the largest possible collections of people (e.g., in the limit, the largest group of all, "mankind"), the PH is interested in applying his knowledge of social science to the study of particular individuals or social groups. In the extreme, the PH is not interested in general theories at all, no matter how humanistically based or grounded they may be. His ultimate concern lies with knowing and helping a particular individual at a particular place and time. His motto is to savor, know, and appreciate the essence of a particular individual. In the extreme again, he doesn't believe that the purpose of social science is to formulate generally valid laws of social behavior, even assuming that such a task were possible. This is not to say it is easy to consistently maintain this position, or indeed, any of the positions we have encountered. As we shall comment on shortly, for all their mutual opposition to one another, each position depends upon and presupposes each other in countless ways. No one of the positions we have been discussing is really self-sufficient.<sup>24</sup> The PH's attitude is strikingly close to that described by Ernst Cassirer in his preeminent study of the structure of mythological thinking:

For [traditional] scientific thought [what we have been calling AS] to "understand" an event means nothing else than to reduce it to certain universal conditions which we call "nature." A phenomenon such as the death of a man is understood if we succeed in assigning a place to it within this complex — if we can recognize it as necessary on the basis of the physiological conditions of life. But even if myth [or, alternately, PH] could conceive this necessity of universal "process of nature," the mythical [PH] consciousness would regard it as mere accident because it leaves unexplained precisely what holds the interest and attention of myth [PH], the death of precisely *this* man at *this* particular time. This individual aspect of the event seems to become understandable only if we can reduce it to something no less individual, to a personal act of the will, which as a free act requires and is susceptible of no further explanation.<sup>25</sup>

It should come as no surprise that the preferred method of the PH is especially suited to capturing the uniqueness of the particular individual or social group he is studying. The preferred methodology is the case-study or, in even more general terms, a deliberately constructed story. As one of the pioneers of this method, William F. Whyte, has put it:

As I wrote case studies of the Nortons and of the Italian community [*Street Corner Society*] a pattern for my research gradually emerged in my mind.

I realized at last that I was not writing a community study in the usual sense of that term. The reader who examines *Middletown* will note that it is written about people in general in that community. Individuals or groups do not figure in the story except as they illustrate the points the authors are making. . . . The reader will further note that *Middletown* is organized in terms of such topics as getting a living, making a home, training the young, and using leisure.

The Lynds accomplished admirably the task they set out to accomplish. I simply came to realize that my task was different. I was dealing with particular individuals and with particular groups.<sup>26</sup>

It is again unfortunately beyond our scope here to discuss the "scientific" status of such a methodology. Diesing has presented the most thorough rationale for the case method.<sup>27</sup> In closing this section, we feel compelled to cite an appropriate passage from Churchman.

The Hegelian inquirer is a storyteller, and Hegel's thesis is that the best inquiry is an inquiry that produces stories. The underlying life of a story is its drama, not its "accuracy" [an AS trait]. Drama has the logical characteristics of a flow of events in which each subsequent event partially contradicts what went before; there is nothing duller than a thoroughly consistent story. Drama is the interplay of the tragic and the comic; its blood is conviction, and its blood pressure is antagonism. It prohibits sterile classification. It is above all implicit; it uses the explicit only to emphasize the implicit. But is storytelling science? Does a system designed to tell stories well also produce knowledge? Or can such a system be "designed"? Or is the storyteller ever a "system"?<sup>28</sup>

### Concluding Remarks

We have tried to outline four very different approaches to social science and to cull out the social psychological roots underlying each attitude. There are many more aspects of each approach than could possibly be dealt with here. Of necessity, we must refer the reader to our book, *The Varieties of Social Science Experience*, which attempts to treat each approach in depth. As preliminary and tentative as we regard our efforts in this area, they nevertheless raise some important questions. If the four approaches we have discussed are "correct," in the sense that four very different attitudes do indeed exist and are in sharp conflict with one another, then what are the prospects, if any, for unification among these four approaches? In short, what are the

prospects for unification in the social sciences if social scientists do indeed invest their psychic allegiance to very different systems of thought?

Interestingly, the answers can themselves be conceived as a function of our four approaches. Thus, the AS's response to the question of the possibility for unification would be, "Yes, but *if and only if* we are able to fashion a single dominant theory of social science capable of subsuming the theories, methods, and concerns of the other three approaches in a precise and systematic fashion." The CT might respond, "Yes, but *if and only if* we are able to develop sufficiently rich 'bridge' concepts between each of the four approaches." The GH might respond, "Yes, but *if and only if* we are able to apply interpersonal or team-building techniques on a large enough scale to remove the institutional and individual barriers (conflicts) which divide social scientists from one another." Finally, the PH might reply that unification, if it can be achieved at all, can only be done on an individual basis; i.e., between two particular individuals. What is needed, then, is an approach (a meta-perspective) which transcends each of the four acting separately. In a previous paper, we have tried to outline such a perspective: Systemic Knowledge.<sup>29</sup> There we try to demonstrate that each of the four perspectives is only a component — albeit a needed one — of the entire process of inquiry. Without their realizing it, each of the four approaches outlined is only able to function because it has presupposed, without its being aware of it, substantive knowledge, methods, and theories from each of the others.<sup>30</sup> The difficult, if not seemingly impossible, task is to translate more effectively what this means into the sphere of educating social scientists and to the design of new institutions for the practice of social science *systematically* conceived.

## NOTES

1. P. Diesing, *Reason in Society, Five Types of Decisions and Their Social Conditions* (University of Illinois Press, 1962); P. Diesing, *Patterns of Discovery in the Social Sciences* (Aldine-Atherton, (1971); J. Hillman, *Re-visioning Psychology* (Harper & Row, 1975); G. Holton, "On Being Caught between Dionysians and Apollonians," in G. Holton, ed., *Science and Its Public: The Changing Relationship* (Daedulus, 1974); G. Ritzer, *Sociology, a Multiple Paradigm Science* (Allyn & Bacon, 1975); and T. Roszak, *Where the Wasteland Ends, Politics and Transcendence in Postindustrial Society* (Anchor, 1973).
2. I. I. Mitroff and R. H. Kilmann, *The Varieties of Social Science Experience* (Jossey-Bass, 1979).
3. L. Hudson, *Contrary Imaginations* (Penguin, 1968).
4. E. V. Morse and G. Gordon, "Cognitive Skills: A Determinant of Scientists' Local-Cosmopolitan Orientation," *Academy of Management Journal*, 1974, 709–723.
5. I. I. Mitroff, *The Subjective Side of Science, An Inquiry into the Psychology of the Apollo Moon Scientists* (Elsevier, 1974).
6. *Ibid*; Morse and Gordon. 7. *Ibid*; Diesing, *Reason in Society*.

8. J. Dewey, *The Quest for Certainty, a Study of the Relation of Knowledge and Action* (Capricorn, 1960).
9. We have cited Maslow for other reasons as well. Although not explicitly represented in Figure 1, Maslow has also proposed a typology of scientists strongly in accord with our framework.
10. A. H. Maslow, *The Psychology of Science* (Harper & Row, 1966), 31.
11. C. G. Jung, *Analytical Psychology, Its Theory and Practice* (Pantheon, 1968) and *Psychological Types*, rev. trans. by R. F. C. Hull, Vol. 6 of *Collected Works* (Princeton University Press, 1971).
12. Jung, *Psychological Types*.
13. C. W. Churchman, *Theory of Experimental Inference* (Macmillan, 1948).
14. D. T. Campbell and J. C. Stanley, *Experimental and Quasi-Experimental Design for Research* (Rand McNally, 1969).
15. I. I. Mitroff and T. Featheringham, "Towards a Behavioral Theory of Systemic Hypothesis-Testing and the Error of the Third Kind," *Theory and Decision*, 1976, 205–220.
16. M. Davis, "That's Interesting!, Toward a Phenomenology of Sociology and a Sociology of Phenomenology," *Philosophy of the Social Sciences*, 1971, 309–344.
17. *Ibid.*, 309. 18. *Ibid.*, 313.
19. P. Feyerabend, *Against Method, Outline of an Anarchistic Theory of Knowledge* (New Left Books, 1975).
20. C. Argyris, "Some Unintended Consequences of Rigorous Research," *Psychological Bulletin*, 1968, 185–197.
21. *Ibid.*; C. Argyris, *Intervention Theory and Method* (Addison-Wesley, 1973).
22. Hillman; W. J. McGuire, "The Ying and Yang of Progress in Social Psychology: Seven Koan," *Journal of Personality and Social Psychology*, 1973, 446–456; W. R. Torbert, *Creating a Community of Inquiry: Conflict, Collaboration, Transformation* (John Wiley, 1976).
23. C. Argyris and D. Schön, *Theory into Practice: Increasing Professional Effectiveness* (Jossey-Bass, 1976).
24. Diesing, *Reason in Society*.
25. E. Cassirer, *The Philosophy of Symbolic Forms. Volume Two: Mythical Thought* (Yale University Press, 1955), 49.
26. W. F. Whyte, *Street Corner Society* (University of Chicago Press, 1943), pp. 322–323.
27. Diesing, *Patterns of Discovery*.
28. C. W. Churchman, *The Design of Inquiring Systems* (Basic Books, 1971), 178.
29. I. I. Mitroff and R. H. Kilmann, "Systemic Knowledge: Toward an Integrated Theory of Science," *Theory and Society*, 4/1 (1977), 101–129.
30. Diesing, *Reason in Society*.